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JOURNAL
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NEW EXPERIMENTAL DESIGNS FOR
PSI RESEARCH

BY MICHAEL SCRIVEN

IN the course of, and since writing a 10,000-word discussion¹ of Soal and Bateman's *Modern Experiments in Telepathy*, a number of possible experiments occurred to me (as they probably did to others, perhaps long before). I thought there might be something in some of them that would lead workers in the field to new clues, although I have not worked them out in enough detail to guarantee value or consistency throughout.

Ideas for New Experimental Research

(1) Would it not be worthwhile to do some experiments in which the subject is asked to give the names of cards in a pack *using a random call-order*? He would thus be asked to say what he thought the 13th card was, then the 2nd card, then the 7th card, then the 23rd card, etc., until the numbers up to 25 were exhausted. One interesting consequence of this would be to see if precognition would have a purely spatial aspect in telepathy experiments; that is, would the precognitive subject have any tendency to score on the $(n+1)$ th card when asked to guess the n th card, or would he merely tend to give the m th card where m was the number succeeding n in the *random* card-order? This experiment would also be relevant to the question whether any type of radiation theory would suffice to explain the results of the ESP work. (I am not thinking of electromagnetic radiation, of course, but merely of what we might call *psi*-radiation.) It also provides a means for judging the extent to which the ESP faculty, if there is such, is singularized; and hence for comparing it with memory. Certainly the task mentioned would produce inferior results in a memory test compared with the ordinary DT method (assuming 'displaced sequence' as well as 'direct' scoring).

¹ *Philosophical Review*, April 1956.

(2) With reference to the experimental design employed by Soal in which the card-agent looks at the order of the 5 code-cards at the beginning of the experiment for a period of n seconds, it would, I think, be important to discover whether the *beliefs* of the card-agent as to the order of the code-cards have any effect upon the calling of the subject, or whether it is the *actual* order, as initially registered on the agent's cortex, that determines the subject's success. We know that in several of the experiments the agent *said* he could not recall the card-order at the end of the experiment; but it would be most interesting to have him write down what he guessed the correct order to be at the end of the experiment in these cases, and more importantly still, in those cases when he thought he *could* recall—putting a blank where he had no recollection at all. One of the consequences of this experiment might be the shedding of some light on the general question of the extent to which the unconscious mind of the agent is accessible to the subject with telepathic powers. This would have special reference to the interpretation of the seance results of non-physical mediums.

(3) A good deal of work could be undertaken, with some chance of showing profit, into the question whether a particular subject (perhaps only when dealing with a particular agent) has a tendency systematically to confuse two or more of the symbols. There might be, for example, 'phonetic' confusion, 'visual' confusion, or cognitive confusion (e.g., more tendency to confuse a horse and an elephant than a horse and a centaur in cases where the agent or percipient thinks of the first two as beasts of burden). To obtain the best information on the association, one would have to begin the study by doing some word-association tests. It would also be very interesting to do some experiments in which the subject's ability to *recall* certain symbols (or certain symbol-combinations) more efficiently than others was correlated with his ESP success on them. This work would again do something towards indicating whether and how far we can push the analogy between memory and ESP. (Certainly the search for the pre-cognitive analogy to the engram should be an appropriate one for trained ghost-hunters!)

(4) If we are ever fortunate enough to discover more than one card-guessing sensitive, then it would be very interesting to see how they behaved when acting as agents *or as experimental supervisors* for each other; in the latter case being armed with a set of indoctrinated beliefs. One of the purposes of such a study would be to see how much there is to the idea of the experimenter as a

controlling influence in the experimental results, and also to see whether the interaction between the two sensitives would produce a change in one of their characteristic methods of scoring.

(5) On the question of whether ESP subjects can be trained to (a) recognize their successes and failures, and (b) improve their scoring rate, it is worth noting that each of these is independent of the other, i.e., we could have (a) without (b) or (b) without (a) as well as both or neither. But the essential feature of any experiment designed to fully explore the possibilities of doing either (a) or (b) is a 'guess-by-guess' differential reinforcement procedure. It would be advisable to try different rewards and different deprivation schedules with the different rewards. The range of rewards is greater than might be thought and would vary somewhat from subject to subject, e.g., the use of pornographic pictures (as in the Skinner experiments with schizophrenics or in Fisk's London experiments) or the use of direct encouragement and disparagement in the case of ego-involved subjects. It may be that the apparent success of the uneducated Spanish children in the recent investigations is not unconnected with the authority position held by the woman who acted as experimenter in the first instance. Again, in an experiment disguised as a perception experiment under poor seeing conditions, such as I believe has been done by a student of Gardner Murphy's, there might be more value to the use of encouragement-disparagement with adults than is the case when their natural scepticism (which is possibly present in almost all subjects whether they explicitly acknowledge it or not) is involved as in the ordinary ESP tests.

(6) A judicious expansion of the interesting series using repeated targets would be well worthwhile. Already these results tie in with certain other theoretical leads, particularly the possibility of psi-confusion between certain symbols relative to a specific agent. There might well be other methods of scoring these than by the means mentioned on pp. 178-80 of Soal and Bateman's book. For example, scoring for the number of repetitions compared with Shackleton's normal rate of call-repeating might give greater significance, especially in view of his tendency to under-call these. A further consequence of doing this sort of experiment would be to encourage the subjects to indulge in longer strings of repeated calls than they normally do, and hence give the ESP 'faculty' less of a barrier to overcome. Even checking up on the original records of the experiments mentioned on p. 179 might yield some evidence for confusion between the original symbols which are not themselves there identified.

(7) The question whether Shackleton's health influences his scoring rate suggests a more general question, viz., does his attitude have a more noticeable effect. Reading through Shackleton's comments on p. 184-5 suggests that the correlation is at least as great with attitudes of a certain kind. Devising a transient attitude scale, as opposed to a personality scale, might provide us with the necessary net of smaller mesh which we require to prevent those elusive fish—the conditions of ESP success—from slipping away.

(8) Some attempt to induce a subject to score below the level of chance would be most important in order to decide finally (?) whether the ESP faculty is producing some information all of the time or all information some of the time. On this depends the validity of the model described in my review. Even if it were not possible to obtain voluntary control of results, it might still be possible to induce negative scoring by the use of reversed reinforcement procedures. It might be worthwhile to consider starting such a training schedule by using repeated target series.

(9) It seems to me there is something to be said for the universal adoption of a mechanical signalling device, such as a buzzer or a light system between the two rooms. This would (a) enable the door to be closed completely and (b) eliminate any possibility that unconscious voice-codes are *assisting* the scoring—granted that they could not wholly explain it under the experimental conditions used (p. 205). Another minor experimental precaution would involve changing Mrs Goldney's procedure in selecting counters for the +2 experiments (p. 154) so as to give repeated targets a random distribution.

(10) With reference to the hypothesis that the agent's unconscious mind contains a record of the actual order of the code-cards, which he examines only at the beginning of the call-sequence (see 2 above), it would be worthwhile testing the hypnotic recall ability of various agents. Certainly if they could recall the actual order under hypnosis, there would be good reason to believe that this data was in some sense available to the sensitive. However, we could not argue from the lack of success in recalling the order under hypnosis to the absence of this information from the agent's unconscious mind, although it would decrease the degree of belief which we should have of this possibility.

(11) With reference to Soal's hypothesis that 'professional mediums and clairvoyants in 90 per cent of cases "read" their sitters' minds, and only very rarely tell them anything which they

do not know, or which is not in their memories', it would again be worth trying to induce hypnotic recall of this data in the sitters. This could not be done in a very conclusive way, since it would be difficult to know in advance of the seance what to dredge up; and after the seance it would be too late to discover whether the material had been 'there' when produced by the medium; but it could be used for certain limited tests. A more direct test would involve the use of amnesiacs.

(12) Any new sensitive should certainly be given an opportunity to test his or her success with clock-cards, since these afford a ready method of scoring 'near-misses' and also a degree of statistical elasticity midway between playing-cards and Zener cards. Against this plan should be registered the following two fairly obvious considerations: (a) that the distinctiveness of different clock-cards is markedly inferior (with respect to most ordinary perceptual cues) to that of the animal cards or Zener cards. It might be worth considering the use of clock-cards having two hands so that an increased degree of similarity to known objects is retained, and thus (perhaps) a greater distinctiveness. The penalty for that would be, of course, that such hours as the hour at which the session commences or at which work ends in the office, or midnight, would have the characteristic 'ace-of-spades' over-strength. (b) Our whole method of scoring misses depends upon the assumption that visual *gestalt* similarity is correlated with psi-similarity. Success discovered by scoring in this fashion constitutes indirect evidence for the latter hypothesis, of course, but failure should lead us to look for other relationships which we may not be able to explain in ordinary perceptual terms. We should look at the matter from a purely mathematical standpoint, asking ourselves whether any transformation of the guess-order would yield an improvement in the 'success' rate—either a many-one transformation, or a one-many transformation, etc.: and we should consider 'content' relationships as mentioned above.

(13) A statistical investigation could be made to determine the validity of Dr Soal's hypothesis that the 'crowding of hits into the first three segments of the run is undoubtedly responsible for the wide divergence of the numbers of runs of 1, 2, 3, 4, or more consecutive hits from the expected values' (p. 314): the actual case being that there is 'a considerable excess of runs of 3, 4 or more consecutive hits' (p. 315). Unless this really is a statistical artifact, then I would be impressed by the possibility that it is evidence for the possibility that the 'noise' in the neural network occasionally falls off in magnitude, i.e., that there are brief periods

of 'contact'. If this is so, we could set about the problem of identifying such periods from other indicators, e.g., from correlation with the recurrence of certain sequences in the target order, or from variations in the EEG, PGR, etc.

(14) In any theory which contemplates a frequent but weak (rather than infrequent but strong) functioning of the psi-faculty, it is important to devise experiments in which the normal stimulus-response patterns including habitual calling patterns are allowed to ossify the call-sequences as little as possible. Professor Meehl has suggested to me that the paradigm of such experiments would involve an agent gazing at some randomly chosen object, scene, picture, etc., while the subject, unaware of the existence, identity, location or activity of the agent, is asked to free-associate, thus being free of all constraints on his responses and *a fortiori* of the powerful tendency to produce a pattern in a series of restricted responses. Freud argued, of course, that in free-association the conscious mind is relaxed, and material from the unconscious slips out into the flow of words. Certainly if such an experiment gave significant results, scored analogously to Whately Carington's, we should have acquired interesting and perhaps useful data. But it might be necessary or preferable to introduce some 'linkage' by means of specific transmitting instructions to the agent as to the location and/or identity of the subject, or the analogous material for the subject, or both. And one might succeed in retaining enough of the merits of these procedures while adding the advantages of slightly simplified interpretation in a modification where the agent would be given what I shall term a conceptually random pack. This would consist in, say, twenty cards, all different in the following basic sense. The first might be a sign of the zodiac, the second a solid colour, the third a symphonic theme, the fourth a number, the fifth a foreign phrase, the sixth a diagram of the light rays through a collimator, etc., such that no two cards in the pack would represent members of the same proximate *class*. A number of such packs could be made up, each of them totally different in the sense of containing no two identical cards but not different in the stricter sense just mentioned. The subject would be told to 'free-associate' for twenty (or sixty) minutes, while attempting to match his train of thoughts to those of the agent next door (unidentified—to prevent standard associations). The agent would turn up a new card every minute (or every three minutes): and this fact plus the fact that the cards are virtually unrestricted in what they represent would also be known to the subject. The possible

advantages of such an experiment would be that it involves a technique which is (1) more sensitive than the Zener or clock cards in the sense that it could detect agents or subjects whose success is limited to certain non-standard types of material; and (2) more comprehensive than the Carington drawings in the sense that a wider range of material could be scanned for success in a shorter time. Of course, it may be *necessary* to spend an hour concentrating on each item to achieve results. But we lack evidence for that view, and the proposed design might give us a great deal more data about the best transmitting material for a given agent-subject combination. A variation would have the agent also free-associating for the entire period, e.g., into a tape-recorder. Advantages: the material to be matched is at the same level instead of being something observed, on the one hand, and someone's thoughts, on the other. After all, we never know what the agent actually thinks about when he looks at a Zener card—the symbol itself may be a very small part of his mental content. Disadvantages: it is—for many people—very difficult to really free-associate for an hour (or even twenty minutes) without a great deal of practice; and there is a sense in which the succession of thoughts in free-association is itself determined by laws, those very laws which Freud thought he understood and which made free-association a valuable tool for psychoanalytic diagnosis. In so far as this is true, one would have merely substituted one source of signal-drowning response patterns for another; but it appears likely to provide a much weaker set. Freud could, and did, contemplate the telepathic origin of some of the analysand's material without abandoning his faith in its exemplification of certain idiographic tendencies. It might well be advantageous to use as judges in such experiments people familiar with the psychoanalytic theory of symbolism.

NON-LOCALISATION AS A MODEL FOR TELEPATHY BY K. G. DENBIGH

It has been said that the slowest and most difficult stage in the development of any major theory, for example the science of mechanics, is the invention of the primary ideas themselves, the elementary notions such as mass and acceleration without which the theory could not exist.

This is surely the stage at which psychical research finds itself

at the present time. Our existing notions do not provide the conceptual framework within which to explain the experimental results obtained so patiently by Soal and Rhine and others.

To this situation there is a very interesting and suggestive parallel in the recent history of physics. As is well known, the origin of the quantum theory was the impossibility of accommodating certain experimental results within the nexus of existing ideas. This required a clearing of the ground in order to obtain greater freedom. A more flexible system was constructed and certain of the concepts of classical physics, in particular 'wave' and 'particle', took on a far less restricted meaning than they had previously possessed.

The purpose of this paper is to suggest that the parallel actually goes much further. To be sure it is only a parallel and it may be deceptive. But the fact remains that the salient experimental effect which is met with in extra-sensory perception, as in the origin of quantum theory, can best be described as a *non-localisation*.¹

In classical physics electrons and protons were thought of as being sharply defined entities, i.e. as being 'corpuscles' or 'particles' in the classical terminology. But in quantum mechanics (or at any rate in all verbal interpretations of phenomena) electrons and protons can no longer be regarded as having a unique location in space and time. All that we can properly speak of are the relative probabilities of these entities being in one region or another and these probabilities are expressible mathematically by means of a wave equation. The 'particle' is at the same time a 'wave' and the wave expresses the probability that the particle may be either *here* or *there*. Thus along with this dualism there is a fundamentally statistical outlook, the result of the impossibility of attributing a precise location.

How similar this is to the state of affairs in psychical research! Or at least to that part which has been most clearly demonstrated, that part known as telepathy. Here we have the situation, to put it crudely and pictorially, that a particular thought or mental image is not uniquely in the mind of one person; that there is a possibility, exceeding chance expectation, that your thought is my thought and also the possibility that the instant (on clock time) at which it is your thought is not the same as the instant at which it is my thought.

¹ This term, as it is used here, is to be understood in a somewhat general sense, i.e. as concerning non-localisation in *time*, as well as in space, or more generally without any necessary reference to physical co-ordinates.

Moreover it has been generally agreed that ESP is not a form of cognition—the card guesser does not *know* that he has guessed correctly. Indeed the whole effect can only be shown statistically ; all that we can do is to assert a probability (which is always low) that your thought will be my thought, or that your 'self' has penetrated, in some sense, into my 'self'.

Obviously the analogy must not be pressed too far. In physics the motion of a particle or the structure of a wave is described in terms of space and time and it is far from my intention to suggest that these co-ordinates (at any rate space) are directly applicable to mental phenomena. The value of the analogy is rather in giving a clue to the kind of theory which may be needed. In physics the notion of the separate entity or 'particle' has broken down and has been replaced by a more flexible concept. Something similar may be necessary in psychology and in particular we may have to try to think of the personality as having some quality analogous to extensiveness or non-localisation.

It may be remarked that the notion of the personality, in the West, has been characterised, like the classical picture of the electron, by a 'corpuscular' quality. It has been thought of on the model of a physical object, sharply defined in space and time and located, as it were, within the living body. Even in regard to survival after death, the same notion has been held ; survival meant survival as an *individual spirit* and not, as in Brahmanic philosophy, by an impersonal absorption into the infinite.

The tenacity and strength of the corpuscular conception of personality lies in the clarity of Me. Thus at the basis of Descartes' philosophy was his perception of himself as an existing being. It is a conception which is greatly favoured by the conditions of everyday life where we habitually maintain ourselves, for purposes of survival, at the highest level of conscious awareness.

But there are certainly quite different states of mind in which, whilst we are busy with them, we are literally unaware of ourselves. These are especially the states of day-dreaming when the mind seems to wander at random from one thought to another, and the attention is diffused and tenuous.

Words, which imply a localisation, such as 'I' and 'here' and 'now', perhaps have a value only as applied to mental states where the attention is sharply focused (states which do, in fact, lead the speaker to use these words). However, if our purpose is to obtain some understanding of ESP, we must recognise the existence of mental states for which these words are much less meaningful. When my attention is relaxed I am no longer conscious of myself as of a sharply defined 'I' ; it is as if my being is expanded rather than

localised. And, of course, it is these conditions of passivity and 'openness' which seem, from what little clear evidence is available, to be the most favourable for ESP. In particular, it is during sleep that some of the most remarkable previsions have been claimed.

In order to give an example where the idea of 'non-localisation' may be usefully applied, I shall turn now to the precognitive effect. Granted the existence of telepathy, in its ordinary form, how do we go forward to explain the successful guessing, at the present instant, of an event which has nominally not yet occurred?

It may be useful to remark, first of all, that there is no purely scientific criterion of what *is* the present instant. The moment which we call *Now* is defined by consciousness, by the attention, and in no other way. This important point seems to have been generally overlooked and is worth discussing.

If someone were to ask me, 'What is the time?' I should look at my watch and report that it is, say, five past four. My evidence for this statement is that the minute and the hour hands coincide now, at the present moment, with such and such numbers on the dial. But if someone were to ask me, 'Out of all moments, how do you know which is the *present* one?' what should I answer?

Briefly our decision concerning the *time* is based on the observation of something external to ourselves—the position of the hands on the dial. On the other hand, the decision that this observation is a *present* observation, and not a past or future one, is made within ourselves and is subjective.

Thus whenever we take a coincidence reading in science, whether it is with the eyes, the ears or any other senses, it is the mind which judges that the reading is actually *now*. Of course this does not imply any ambiguity or uncertainty about the *t* co-ordinate of physics, where we regard events as being extended over a temporal continuum. But the decision that *t* has reached a certain value, i.e., which one is the 'now' instant along this co-ordinate, is based on a subjective assertion that a certain state of the observed instrument is its *present* state.

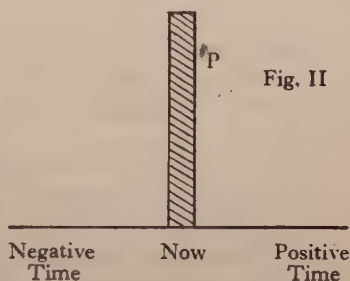
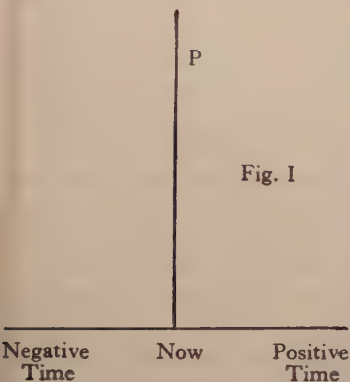
What I shall now go forward to suggest, using an extension of Saltmarsh's theory,¹ is that the psychological 'now' does not have a pointlike quality at all levels of consciousness—it is not sharply 'localised' along the co-ordinate of physical time.

Let it be supposed that I look at my watch and report that the time is 4 hours 5 minutes and 10 seconds. My ability to state

¹ H. F. Saltmarsh, *Proc. S.P.R.*, 1934, 49.

the time to within a second, or better, despite the fact that the hands are moving continuously over the dial, is based on the fact that my attention seems also, *for most purposes*, to have the quality of a moving point. The intensity of my perception has a peak at what I call the present moment and this allows me to say that the hands of the watch have a definite position.

Is this peak perfectly sharp? Let it be supposed that the intensity P of perception (if it were measurable) is plotted against time, choosing the zero of time (i.e. what we call *now*) as coinciding with the peak. If the latter were perfectly sharp it would be as represented in Fig. I.



However, it is known that this kind of picture does not give a correct account of the psychological sense of time in the presence of something which is changing rapidly. The movement of a shooting star, for example, is seen as an instantaneous whole. Therefore psychologists have put forward the view that our sensation of the present moment covers a finite duration (of the order of a fraction of a second) and is certainly not an infinitesimal. This state of affairs is indicated in Fig. II, the width of the shaded portion representing the duration of the 'specious present'.

This concept of the specious present, as it stands, is insufficient to explain the precognitive effect in telepathy. In Soal's work with Basil Shackleton it was shown that the percipient was able to make successful guesses of events as much as two seconds before they had nominally 'occurred' (and also, in the postcognitive effect, two seconds after they had 'occurred'). This is much greater than the duration of the specious present, as it is understood in psychology.

On the other hand our knowledge of the specious present is so incomplete that it leaves many questions undecided. We may ask, Does it join on to the past and the future discontinuously or not? *As scientists we shall prefer to believe in continuity, whenever the evidence is not directly against it.*

Therefore it is perhaps not unreasonable to suppose that the intensity of our perceptions should be represented as a *continuous* function in the temporal co-ordinate, rising to a smooth peak at what we conventionally call the present instant, but tailing off both in front and behind. This state of affairs is represented in Fig. III¹ and it indicates a finite possibility of perception outside the present.

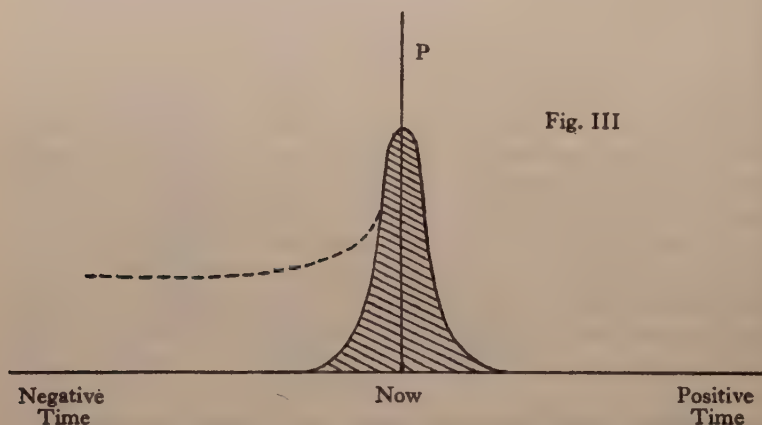


Fig. III

In brief, the view which is being put forward is that 'now' is not as sharply localised at all levels of consciousness as it is for the fully conscious attention. In some of its aspects the present instant is to be thought of as pointlike and sharply defined, like the particles of classical physics; in other aspects it is perhaps more like an advancing wave front, or a wave packet. To be sure

¹ The dotted line in Fig. III shows an alternative representation of the intensity of perception *behind* the peak, in the form of *memory*. As remarked above we have no scientific criterion which can decide for us that the present moment *is* the present moment—it is only ourselves who can do this. And to me an event of an hour past can be said to be past (and not present) simply because the intensity of my perception of that event is fading in my consciousness. This suggests that memory might be regarded as a diminished intensity of perception behind the peak. As this is outside the context of the present article it is not developed in detail.

these are only analogies and very imperfect ones, but they may provide the essential clue to the understanding.

Consider the situation in card guessing experiments in telepathy. At a certain instant, which the agent, the percipient and all others agree is 'now' (on the basis of the pointlike attention) the agent looks at a card and the percipient makes his guess. It turns out that, over many thousands of such guesses, the best scores may be obtained, not on the 'target' cards, but on those which the agent inspected about two seconds earlier than 'now', or those which he will inspect about two seconds later than 'now' (post-cognitive and pre-cognitive effects).

Granted the existence of telepathy between two persons, the additional phenomena of the temporal displacement can be explained, if it is supposed that the specious present is of longer duration at lower levels of consciousness and if the information which is obtained at these lower levels can occasionally be transmitted into full awareness.

This is essentially the theory put forward by Saltmarsh and it has been criticised by Soal and Bateman¹ on the grounds that 'it leaves unanswered how we become aware of an event which has not yet happened'.

The new points which I have put forward are, on the one hand, the idea of 'non-localisation' as a physical analogy, and on the other (in reply to Soal and Bateman), the view that there is no objective criterion for the statement that an event 'has not yet happened'. In the human individual there is a sequence of 'now' states and in the external world there is, or seems to be, a sequence of events; we can say that certain events are taking place 'now', or that others 'have not yet happened', only by reference to the subjective criterion of what is 'now'. In the ordinary affairs of life this leads to no ambiguity because the intensity of perception, our awareness of what is 'now', has normally the quality of a moving point. This enables us to say with reasonable certainty that a particular event 'has not yet happened'. (And it may be remarked that this conviction is reinforced by the fact that your reported 'now' coincides with mine.)² However, it may be that the intensity of perception at the sub-conscious level is not sharply localised as a point, but on the contrary is a *continuous* function in the temporal co-ordinate. If so, the conventional 'now' becomes

¹ S. G. Soal and F. Bateman, *Modern Experiments in Telepathy*, London, 1954.

² Of course it is an important fact of observation that we all normally agree on what is 'now'. If it were not so, we should not have the same strong conviction of an external reality moving with us.

simply the region at the peak of this curve. The statement that an event 'has not yet happened' then loses some of its operational significance; for whereas the conscious perception operating at the peak reports that the event has not yet occurred, the unconscious perception operating beyond the peak reports that it has. Or at least what it achieves is a guess, more often right than would be expected from chance, concerning how the event will turn out to be—and, of course, the fact that we are concerned with probabilities, rather than with certainties, is not out of line with our physical analogy of 'non-localisation'.

A CASE OF CLAIRVOYANCE?

WE are indebted to Mr G. Zorab of the Dutch Society for Psychical Research for the following account of an experience which befell Mr J. van Bussbach. Mr van Bussbach (abbreviated to 'v.B.' in the report), an Inspector of Schools in Amsterdam, lives in Delft, ten miles away. He is a member of the Investigation Board of the Dutch Society.—ED.

Towards the end of 1954 v.B. sent a large parcel of documents to the Dutch Mathematical Centre in Delft. It contained the scoring sheets of his ESP experiments with schoolchildren which were to be rechecked independently at the Centre. On 30 April 1955 v.B. wrote to the Centre asking them to return the documents as the rechecking had been completed.

On 12 May 1955 the Centre answered and stated that the documents asked for had been posted to v.B.

On 20 May, however, v.B. wrote back that he had indeed received three parcels containing documents, but that he was sorry to say that the series 100-596 and 1605-1677 were missing. As these papers were not among the documents returned, would the Centre be so good to mail these sheets to him.

In a letter dated 27 May the Centre expressed their surprise that the documents mentioned by v.B. were not among the other papers sent him. They would, however, search for them, but were at a complete loss to understand that such a large batch of papers could have been mislaid or gone astray. They urged v.B. to look for them again in his own home.

On 5 June v.B. answered that he had looked everywhere at home but could not find the papers. He was very worried, as

the documents were very important to him, and therefore asked the Centre to search the whole office for them.

Answering v.B. on 13 June the Centre declared that they had now gone over practically every inch of their office without finding any trace of the lost documents. The Centre then suggested that they might have handed them over to Dr S. T(romp), who had acted as intermediary between v.B. and the Centre.

On 13 July another letter was sent by v.B. to the Centre with the urgent request to look for the papers, as he was absolutely certain these were not at his home.

On 15 August v.B. wrote to Dr S.T. asking whether he knew anything about the missing papers. On 11 October Dr S.T. informed v.B. that Mr S., one of the members of the staff of the Centre, had telephoned him to say that they had gone through all the dossiers of the Centre but could not find the slightest trace of the missing documents. Dr S.T. ended his letter by saying: 'If you are acquainted with Mr Croiset [a well-known Dutch clairvoyant], why not ask him to try to find out whether the documents have been destroyed, or if not where they can be found at the moment?'

I (Zorab) am witness to the fact that from May 1955 onwards v.B. was very worried about the lost documents, and at several meetings of the [Dutch] S.P.R. Investigation Board complained that he would be at a complete loss what to do if he had to produce the material on which he had based some articles in a foreign parapsychological journal.

STATEMENT BY v.B.

On 10 October 1955, at about 10 a.m., I phoned from my home at Haarlem Mr G. Croiset, living at Utrecht. Mrs Croiset answered the telephone, and said that her husband is too busy at the moment with his work of healing to come to the telephone. 'But what is it about?' she asked v.B. 'Well', says v.B., 'I want to consult your husband about some missing documents.'

Mrs Croiset: 'For such things he has no time. Go, and ask one of these telepathic sensitives.' v.B.: 'Now, Mrs Croiset, please tell your husband that v.B., from Haarlem, is on the line. He knows me, and is sure to come.' This happens, and Mr Croiset coming to the phone asked v.B. what he can do for him.

v.B.: 'The documents relating to my second ESP experiments with schoolchildren, about 600 sheets of paper in all, were sent to the centre at X. These documents, however, cannot be found anywhere. Of one thing I am absolutely certain, viz. that they have never been returned to me. At the Centre the staff have looked in every possible place. . . .'

Mr Croiset: 'Just one moment. A writing-desk chair, a revolving

one, with three legs ; a writing-desk with a green top. Can you place that?’

v.B. : ‘I do not know anything about it.’

Mr Croiset : ‘In that room are two cupboards, high cupboards, of a brown colour. In one of them your documents are to be found. In the one to the right. Don’t worry, you will get your papers back all right.’

After these words the phone-call came to an end in the usual way. During our conversation I had noted down every word Mr Croiset had said.

11 October 1955. At about half past 10 a.m. I entered the office of the Centre at X. Mr H is not present ; Mr M., one of the chiefs, however, attended to me in his own room. The chair before his writing-desk was of the revolving type, turning round on a four-legged frame. The chair’s upholstery, as well as that of all the other chairs in the room, was of a green colour. In the room, to the left and to the right of the writing-desk, two high cupboards could be seen, made of a grey-coloured metal.

We started talking about the missing documents, and Mr M. expressed his deep regret that v.B.’s papers could not be found anywhere in the Centre’s office, though every possible spot had been searched with the greatest care. Mr M. also said that he seemed to remember, though very faintly, that I had taken the documents away with me several months ago. The possibility of this was emphatically denied by me, and I pointed out that I could not have possibly carried away such a heavy and cumbersome parcel containing 669 sheets of paper.

I then started talking about what Mr Croiset had said to me over the telephone re the lost documents, and began comparing the data given in that conversation with the way in which the room was actually furnished, etc. Mr M. was of opinion that the top of his writing-desk was black, but to me it looked as if there was a greenish glow in the colouring of the desk.

I pointed to the high cupboard on the right-hand side of the writing-desk.

Mr M. : ‘Impossible! The documents cannot be in that cupboard. Nobody is allowed to open that cupboard. And it is not according to the regulations to have papers such as you sent us shut away in that cupboard. I am, therefore, absolutely certain that the documents are not in there. However, just to satisfy you, we shall have a look.’

Mr M. opened the door of the cupboard, and the first thing to catch his eye was the pile of documents lying on the left-hand side of the top shelf. It was a complete mystery to him how the papers had come into that cupboard.

On 14 October (evening) v.B. wrote a letter to Mr. M., and enclosed the above statement. He requested him to hand him (v.B.) a signed statement, saying that he was in agreement with

v.B.'s version and description of what had occurred during the interview in Mr M.'s room re the finding of the documents.

STATEMENT OF MR M. (DATED 19 OCTOBER)

I am in complete agreement as to the essential points of what happened on the morning of October 14. The following details, however, I would like to add.

(a) In the room mentioned two similar writing-desks are to be found, both showing a black top. Behind my desk stands the described revolving chair. Behind the other writing-desk is an ordinary (steel) desk-chair with a green seat and back. There are also in the room two large tables with black tops and six chairs.

(b) The documents were not the first thing to catch my eye. After having glanced around, I began to search through the cupboard systematically. To do this I naturally started to look at the lefthand-side of the top-shelf first thing, where the documents happened to be.

(c) It is not improbable that the documents were placed in this cupboard owing to the following circumstances. About six months ago the two cupboards mentioned by v.B. were put into my room. All books and papers had to be put into these cupboards. For this purpose all books, etc., were sorted out on large tables. On these tables presumably the pile of documents were placed, as the sheaf was too large to be put into the smaller document-lockers. As in the cupboard on my righthand-side all annual reports, theses, unbound books of a non-statistical nature, and also mimeographed reports, very much like your papers in appearance if looked upon edgeways, are placed, the presumed explanation is that the documents were put into the cupboard together with a sheaf of reports on which the v.B.'s papers were lying.

This case is typical of the many whose assessment depends entirely on the reader's personal judgment. On the 'credit side' one may put the correct statements that the papers were in the right-hand cupboard, and that the room contained two cupboards, a revolving desk-chair, and a writing-desk. On the other hand, Mr Croiset mentioned *a* green-topped desk: there were two, more black than green; a three-legged revolving chair: it was four-legged; brown cupboards: they were in fact grey; and did not mention the two large tables, the ordinary (steel) desk-chair, and the six other chairs. Mr van Bussbach had told him that the papers had been sent to the Dutch Mathematical Centre and that he was sure they had not been returned to him. The inference that they might still be somewhere in the office would therefore not be hard for anyone to draw; and Mr Croiset's description would apply to a good many offices.

It is, of course, important to know whether Mr Croiset had ever

been to the Centre. On this point Mr Zorab informs us that Mr van Bussbach has stated that he is 'very positive that Croiset had not been there' and that 'only a few members of the Dutch S.P.R. Investigation Board knew that van Bussbach had contacted this Centre in order to have his results verified.'—Ed.

'SOMETHING TOLD ME TO GO ON TO VICTORIA ...'

FOR this report we are indebted to Mrs Frank Heywood, a member of the Council of the Society, who was informed by telephone shortly after the experience had taken place, and at whose instance it was recorded without delay.

As I had an appointment with our dentist today at 12.30 p.m. I arranged with my husband Jan at breakfast that he should have his luncheon in the Admiralty canteen instead of in our flat. I also had to buy something for dinner at a shop opposite Gloucester Road tube station and thought that I could not be home again much before 2 p.m.

Contrary to my expectation, the dentist only completed one filling (a tooth which had a temporary dressing in it) so I found myself on an Underground train shortly after 1 p.m., having done my shopping.

When the train stopped at Sloane Square station, where I normally get out, for our flat is about three minutes' walk from there, something told me to go on to Victoria [the next station] and there to catch a No. 11 bus back home. I 'knew' that if I did so, I would sit down next to Jan, midway along the bus, on the left-hand side, downstairs.

At Victoria I had to wait about five minutes for a bus. The first one that came along was a No. 11 and I got in and sat down next to Jan—midway along the bus on the left-hand side downstairs. The bus was full with the exception of that one seat. Some people waiting behind me in the queue went upstairs.

Jan was surprised to see me and had had no impression that we would meet as we did. He told me that he had gone to the canteen early so as to be able to return home and collect our wireless, which is broken, and take it to be repaired before returning to the Admiralty.

(Signed) PEARL KOWALEWSKA.

17th November, 1955

The following additional notes were written separately and independently by Mrs Kowalewska and her husband at Mrs Heywood's house two days later :

When I sat down beside Jan he was reading a paper. I tickled his knee. 'Hello!' I said.

Jan : 'Hello!'

Pearl : 'I knew this was going to happen.' (I then explained what had taken place in the tube.) I asked Jan if he had had any similar impressions. He assured me that he had not, but that he had not hurried to catch a previous bus as he would normally have done, and had not caught a No. 25 to Victoria, but had waited for an 11.

I said to Jan : 'You see, there is something in it.'

Jan laughed.

We then started to examine a jigsaw puzzle Jan had bought for me.

(Signed) PEARL KOWALEWSKA.

19th November, 1955.

Pearl sat right by me, on the free seat. I was reading a paper and did not see who it was.

Then she put her hand on my knee—I was astonished and angry—who is it to behave in such a familiar way?

Then I looked who it was—and it was Pearl, suppressing laughter.

I was astonished that I did not know what to say.

Pearl was very excited and gay, and said : 'You see? I knew I will sit by you on the bus. I did not go out from the Underground at Sloane Square station, and I went as far as Victoria, because I knew that I will sit by you in the bus.'

(Signed) JAN KOWALEWSKI.

19th November, 1955.

THE BORLEY REPORT : SOME CRITICISMS

BY MICHAEL COLEMAN

It was with great interest that I read the recent report of Dr Dingwall, Mrs Goldney and Mr Hall (1). Whilst I should like to take this opportunity to congratulate the authors on the energy and enthusiasm with which they have prosecuted their investigations, I feel obliged to call attention to certain points in their report which raise difficulties with respect to some of their conclusions.

These may be considered under various headings thus :

- (a) Accusations of deliberate trickery on the part of the late Harry Price.
- (b) Criticisms of his methods of investigation.
- (c) Criticisms of his reporting.

In addition, I should like to draw attention to the status of evidence from other observers, and discuss its implications.

(a) ACCUSATIONS OF PRICE'S DELIBERATE TRICKERY

(i) *The Stone-throwing Incident*

Perhaps the most serious accusation against Price in the whole report is that derived from the evidence of the reporter Mr Charles Sutton. Now whilst Mr Sutton's original accusation (2) is quoted on p. 31 and further material is quoted on p. 71, in neither of these quotations is there an explicit statement that Sutton actually saw Price throw the stone. On the other hand, Lord Charles Hope's testimony, quoted from undated notes (and apparently compiled from memory), states that Sutton actually saw Price throw the stone. It seems inconceivable that Mr Sutton remembered the details of the occasion but forgot the most striking incident, viz. the sight of Price throwing the stone, if he did actually witness this. Again, if he did catch Price 'in flagrante delicto', why do the authors of this report not quote his first-hand testimony to this effect, rather than the second-hand testimony of Lord Charles Hope? That Lord Charles Hope's not being invited to assist further at Borley was not necessarily due to his suspicions concerning this incident (p. 33), will be obvious when his disagreement with Price over the mediumship of Rudi Schneider is recalled (3).

(ii) *The 'Wine-to-Ink' Trick*

As I propose to examine Mrs Smith's evidence later, I shall not here consider the occasion when this phenomenon is alleged (by Mrs Smith alone) to have first occurred. On the second occasion of this occurrence Mrs Goldney was herself present (p. 60). It seems strange therefore that we receive no first-hand impressions of the incident from her. It seems reasonable to expect some comment: for example, did she agree with Price that Mrs Foyster was responsible for the trick? On p. 93 we are told that Mr Foyster omitted this incident from his account deliberately, with the suggestion that he suspected his wife of being responsible for the effect. On pp. 60-1, however, the authors are at pains to argue that Price, from his knowledge of conjuring, must have been the responsible person. On p. 92 we have a curious criticism of Mr Foyster's testimony: 'But this admission scarcely justifies the inclusion in his narrative of events which would appear to the normal observer to be clearly related, without apparently his recognising the connection or commenting upon it.' It is not clear what is meant by a 'normal observer' here, unless it refers to someone who was *not* deeply in love with Mrs Foyster. It seems unreasonable to expect a reporter who does not recognise

the connection between two related events to comment on their connection.

iii) *The Smashed Vase*

On p. 70 we learn of the incident of the smashed vase, with the implication that Price himself threw it. This interpretation depends largely on the *present* account of Mrs Smith (whose testimony I shall examine later in greater detail). Her account to Mr Glanville in 1937 exonerates Price because she then said the incident occurred before his arrival ; a statement in agreement with Price's own published account. The statement of Mr Wall does not rule out the possibility that the other vase was already smashed, as Price, and Mrs Smith (in 1937), described.

(b) CRITICISMS OF PRICE'S METHODS OF INVESTIGATION

i) *Omission of Psychical Researchers from Observers*

This seems to me a curious criticism. I feel fairly confident that the number of people who have had experience of investigating one haunted house, not to mention two, must be extremely small. It seems possible to me that the inclusion of psychical researchers might raise difficulties in connection with the criticism below ; viz. previous reading would 'suggest' phenomena. In any case to attribute their omission to a deliberate plan (p. 5) seems to be going further than the evidence warrants. The same complaint of the employment of inexperienced observers is made on p. 169.

ii) *Issue of the 'Blue Book'*

It is difficult to see exactly what offends the authors in the quotation from the 'Blue Book' reproduced on p. 125. The injunctions to observe and record carefully seem to me unexceptional. Are the authors of the 'Borley Report' as sceptical of 'hosts' as they suggest the members of the University of London Council were? Would they regard *Phantasms of the Living* (4) or Tyrrell's *Apparitions* (5) as useless reading? What approach could they adopt if confronted with what they had reason to suspect was an apparition? If apparitions and quantitative research are to be ruled out of court, the scope of psychical research would indeed be limited!

Price's appearance in the 'Blue Book' as Hon. Sec. of the N.L.C.P.I. testifies to his love of honorific titles, but surely nothing more. On p. 127 it is suggested that reading about past phenomena and the instruction to watch for the 'nun' at the place at

which she was alleged to appear were responsible for Mrs Lloyd Williams's observation. Are we then to conclude we must neither read of psychical phenomena nor visit the sites of their alleged occurrence? I feel such a conclusion would restrict the activities of psychical research to an unreasonable degree.

(iii) *Lack of Instruction to Observers*

On p. 138 we find the authors complaining that Price 'left observers to fumble around as best they might'. Similar charges are made on p. 169, where the fact that Mr Glanville deplored the absence of a common log-book and the consequent lack of information to observers is quoted with approval. This seems a little inconsistent with the foregoing criticism, that detailed instructions and descriptions of past phenomena are responsible, by suggestion, for the observations recorded.

(c) CRITICISMS OF PRICE'S REPORTING

(i) On p. 4 our authors take Price to task for denying that any of the Bull family were interested in spiritualism at the time. They quote Price as saying that Henry Bull used to 'commune with the spirits', and then deny this on the authority of his son and daughter.

(ii) On the same page they reprove Price for his omission of reference to the possibility of rose-bushes tapping in a 'ghostly' fashion on the windows. They admit, however, that he included a warning on this subject in his 'Blue Book'. Since some selection was inevitable from the mass of material available to him, it can hardly be held against Price that he did not include reference to this subject. If there was good evidence that such tappings had been reported as ghostly phenomena, their censure would indeed be justified, but this does not appear to be their contention.

(iii) On p. 19 Price is accused of manufacturing several incidents from a single experience of Miss Ethel Bull, and yet the account given to Mr Hall :

The man I saw once standing beside my bed was tall and dressed in dark clothes, it was twilight, and once or twice I felt someone sitting on the side of my bed.

is not unambiguous. It could well be interpreted as referring to more than one occasion. It is a pity that Mr Hall did not secure a more explicit statement from Miss Bull.

(iv) The admission on p. 20 that Miss Bull may have mentioned a 'headless man' seems to rob 'the most striking example of exaggeration' of much of its point. This is another example in

which the authors could have sought the testimony of Miss Bull to good effect.

(v) The authors several times assert that all mention of rats and mice has been excluded from Price's published works. Thus on p. 49, footnote 2(b): 'We have shown that in his books Price denied the presence of these rodents in the house. . . .' Yet if we turn to p. 162 of *The End of Borley Rectory* (6) we find in the report of Messrs Heap and Longmuir '... creaking and chirping noises which we put down to mice. . . .'

(vi) Bad faith is imputed to Price on p. 77 for his removal of the last sentence of Chapter II of his *Confessions of a Ghost Hunter* (7), but his own explanation of its removal seems a very reasonable one.

(vii) The statement that Mr and Mrs Foyster were regaled by stories by the Misses Bull (p. 91) is an assumption and scarcely warrants the implied criticism of Price's statement that they 'knew very little about the things that were alleged to be happening here'. Indeed, Price's statement receives some measure of confirmation by a quotation from p. 116: '... the absence of objective phenomena at Borley ... [is] ... a fact which may be presumed to have been outside Mrs Foyster's knowledge.'

(viii) Since they choose to reprove Price for describing the wall-writings at Borley as unique in *MHH* (8) the authors should in justice record that he draws attention to similar material in his *EBR*.

(ix) On pp. 111-12, after deploring Price's suppression of the 1937 'Marianne Light Mass Prayers' message, the compilers accept Price's statement that no messages appeared after 1931 and quote Mr Foyster in support of this. Mr Foyster can hardly be held to provide authority for events occurring (or not occurring) after he had left the rectory.

(x) P. 122. The authors hint at a sinister significance in Price's omission of the bell-ringing incident (when Mrs Foyster was under observation) from his second book. Surely it is not unreasonable to suggest that its inclusion in the first provides an adequate excuse for its omission from the second.

(xi) Perhaps the most serious accusation, in respect of his reporting, is made against Price on pp. 162-5, where the incident of the 'flying brick' is discussed. The incident is certainly a curious one, and even Mrs Thompson's evidence does not entirely dispose of all the difficulties. It seems to me from a study of the photographs and plans that the brick is located in mid-air above a point approximately indicated by the second 'S' of the words 'KITCHEN PASSAGE' on the plan of the ground floor. If we

accept the contention on p. 163 that the brick is fellow to those seen in the bottom right-hand corner of Plate XXIII, we may infer that it is on its way to join them. This implies that it was thrown from the left-hand side of the passage from some point further inside the rectory. If we are to accept Mrs Thompson's statement (p. 164) that the workmen were 'concealed behind a wall', a glance at the ground-floor plan shows that the only place where they could stand, so concealed, is in the courtyard. But in her earlier statement Mrs Thompson says, 'All three of us saw him [the workman] as we passed the house. . . .' It is difficult to see how any of them could have seen the workmen in the courtyard, when they were passing the house. It may be noted here that the 'brawny workman' of her earlier (1949?) account becomes several workmen in her later (1950) statement. It would have been desirable to obtain the evidence of Mr Scherman, as the only other surviving witness of this incident, but the report gives no indication that any efforts were made to secure his testimony.

We now come to a discussion of the evidence of Mrs Smith. The authors of this report have set forth the difficulties here raised to such good effect that it seems almost superfluous to raise them again. But the fact remains that the evidence of Mrs Smith figures prominently in their case against the genuine character of the Borley manifestations, and of Harry Price's conduct of the investigation.

We are told on p. 44 that she is a 'practical, sensible woman'. From her own statement we learn that all her life she had been possessed of a 'sixth sense of intuition or perception'. Presumably it is this faculty which provides her with the basis for her speculations on Price's present condition in the Beyond. The authors of the report very properly draw attention to discrepancies between what Mrs Smith now says and her letters of the time. Thus on p. 49 she says that she 'was convinced someone had been waiting outside to play this trick', i.e. closing the library shutters. And yet twelve years previously she had told Mr Glanville (p. 54) that 'they regarded [the sounds] as due merely to the wind blowing [the shutters] until that night when they were pulled together, seemingly by no-one'. That Mrs Smith had no recollection of meeting either Mr Whitehouse (p. 53) or Mr Glanville (p. 57) is certainly curious, and suggests such a psychological mechanism as the authors postulate. It scarcely adds to our confidence in the reliability of Mrs Smith's memory, however. If the rector and his wife were 'led to suppose that he [Price] himself was producing some of the effects' (p. 45), Mr Smith's statement 'some-

how we don't feel safe without you' (p. 51) to Price; and his regret that Price was not accompanying them (in 1938) expressed to Mr Glanville (p. 54), both read rather oddly. That the assertion on p. 55 that Mrs Smith 'did no more, perhaps, than glance at [Price's book] and destroy the four copies sent her' must be accepted because there would be no point in her saying so, if it were not true, is not very convincing. Mrs Smith now says she destroyed the books 'in scorn', but this is at variance with her letter of 2 October 1940. The scorn seems to be a product of her present views, and has been retrospectively applied to her attitude in 1940. The foregoing serves to emphasise that Mrs Smith's attitude today differs from her attitude of earlier years, and that there is reason to suppose that her present views have tended to colour her recollections of past events. I would like to draw attention here, in the light of the authors' remarks on the subject of suggestion (when referring to the 'Blue Book') to the letter of Mrs Goldney to Mrs Smith reproduced, in part, on p. 56. The phrases, 'Did you feel certain *at the time* that Borley was NOT haunted', 'an apparent expert like Mr Price', 'unexplained "phenomena" when he DID arrive', all seem to be very suggestive. Well may Mrs Smith have replied 'Yes!' to such a catechism!

It does not seem adequately demonstrated why Mrs Smith's testimony of what she asserts Mary Smith then thought of the manifestations (p. 65) is to be preferred to Mary's own testimony, at the time and today. Mrs Smith's explanation of the mysterious lights seen in the rectory windows is quoted on p. 45: here she states that she definitely found that they were caused by reflections from passing trains. Since these lights evidently persisted for periods of several minutes, and no-one has suggested that they exhibited any flickering effect (which a moving train would certainly produce) this implies the trains responsible would have to stop near the rectory. How, then, did no-one observe them? The railway is approximately half a mile away, so it is difficult to see how the noise of an approaching train would escape observers in the rectory garden. From enquiries made locally I learn that there is only one signal, a '*distant*' signal on the stretch of single-track line passing the rectory. It is located some 925 yds. from Long Melford box, just north of the Borley crossing. Mr C. E. Cadman, the Stationmaster of Long Melford Station, to whom I am greatly indebted for information in this connection, informs me that trains have only stopped on this stretch of line during a period of flooding some six years ago. Not content with one explanation, however, on p. 48 we learn from Mrs Smith that the lights are due to reflections from the landing above the kitchen.

But an inspection of the upper floor plan shows that no such reflection could be transmitted from the single window of this passage to either Room 7 or Room 11. (As if two positive explanations of these lights are not enough, we have, on p. 139, a third, in which they are attributed to reflections of the lights of Sudbury.) The statement that 'We must accept Mrs Smith's explanation' made on p. 64 requires, at least, a qualification to indicate which of her explanations we are to accept. Our valuation of Mrs Smith's evidence is not enhanced by such innuendo as 'a red pencil was seen next day in the vicinity of Mr Price' recorded on p. 47.

From such considerations as these it seems wise not to make too sweeping an assessment of the phenomena on the basis of Mrs Smith's evidence.

Another person whose testimony requires consideration is Dom. Richard Whitehouse. It is the contention of the compilers of this report that he was referring to the incident of the tumbler when he said he 'did not actually witness [the event]' (p. 102). But is this interpretation entirely reasonable? Dom. Richard is at pains to say he did not witness one of the two events in question; surely this must mean that he did witness the other. We know he could not have been present at the precipitation of the objects from the bedroom since this was the occurrence which caused him to be sent for. Surely, then, he did see the tumbler phenomenon, and has confused the numbers of the two events. (This is not difficult, since they are numbered in reverse of their chronological order.) Such a slip need hardly destroy our complete confidence in the narrator's integrity. After all, the authors are prepared to accept Mrs Smith's evidence in spite of obvious contradictions, and even their present report is not entirely free of errors, but I have no intention of doubting their reliability as witnesses.

I propose to conclude these considerations of individual testimonies with a brief reference to that of Mr A. J. B. Robertson. On pp. 152-3 the authors are at pains to establish that hoaxing is the probable explanation of the lights reported on 30 April 1944. It is remarkable, therefore, to read they agree with Mr Robertson that 'it is difficult to draw any conclusions from these shapes and lights'. Another point worthy of note (on p. 153) is their surprise at the inclusion of various noises in Mr Robertson's reports. On p. 155 of *EBR* Mr Robertson expressly states: 'In investigating a subject where there is great doubt, such as that of haunting, one must carefully record all curious phenomena, even though the events may be due to natural causes.' A remark which immediately follows this suggests an alternative approach to that of the present report: 'In any given case a decision is difficult; it is

therefore of importance to consider the sum total of all possible phenomena.'

To conclude this section, I should like to comment on one or two isolated points. First, the opinion of Dr Davies quoted on p. 123 deserves attention, but can hardly be considered conclusive. In this connection, attention might be drawn to the examination of the Blavatsky-Coulomb letters by the calligraphic expert Mr George Netherclift, and by Mr Sims of the British Museum (9). Both these gentlemen at first asserted that the writings submitted to them were *not* the work of Madame Blavatsky; when they examined further examples, however, they both reversed their opinions. The wall-writings at Borley seem to constitute a very small sample upon which to form a definite opinion.

Since the throwing of stones, etc., has been attributed to Harry Price and/or Mrs Foyster, the authors are obliged to dismiss the account of Lieut. Nawrocki (p. 148) as 'an elaborate hoax of some sort'. But it must be observed here that the Lieutenant would appear to be a man of some scientific training; in the absence of the original testimony, however, too much stress cannot be laid upon this evidence.

Captain Gregson's expressed intention (p. 146) of returning to Borley Rectory does not necessarily conflict with his broadcast suggestion of its haunted character. Indeed, his phrase 'We . . . definitely like the atmosphere at Borley (both physical and otherwise)' seems intended as an antithesis, and serves as a pointer to Captain Gregson's opinion on this matter.

The assertion (p. 160) that either failure to excavate (at Canon Pythian-Adams's instigation) or failure to make any discovery, in the event of excavation, would result in 'The End of Borley Rectory' seems to me unwarranted. Are the authors seriously suggesting that the omission of the Canon's analysis would appreciably affect the sales of such a book? It should be noted that Price's bibliography does not purport to be 'selected' (p. 144); his request for *any* additional material clearly implies that it is intended to be exhaustive rather than selective.

It is now time to consider some of the implications of the items discussed above. First I would suggest, in view of the serious nature of the charges levelled against Harry Price, that it is to be regretted that unequivocal evidence is not forthcoming concerning several incidents for which it might reasonably be expected. Secondly, when considering the suspicions of various persons concerning certain ostensibly paranormal events, it might be well to

remember the remarks of Baggally, Carrington and Feilding in connection with their investigation of the phenomena of Eusapia Palladino (10). Here the authors draw attention to the tendency to 'explain away' as fraudulent, events which they had witnessed under unimpeachable conditions; they preferred to doubt their senses after the event, rather than to accept the implication of what they had observed. Frank Podmore, surely the most sceptical critic of physical phenomena, considered it necessary to advance the theory of a type of 'post-hypnotic suggestion' to explain fraudulent mediumship (11). The suggestion (p. 175) that Price wished to 'bolster up' a few genuine observations with additional material seems hardly adequate, since he had already recorded, in his previous books, virtually all types of phenomena encountered at Borley.

In conclusion, I should like to point out that the material considered here has been almost exclusively drawn from the report as published. On the one hand there is additional material in Price's two Borley books and elsewhere which conflicts with some of the evidence and conclusions of the Borley report; and on the other, there may well be other evidence, accessible to its authors, which has assisted them in arriving at their conclusions, but which they have been unable to print, for one reason or another. Lacking access to this material I have necessarily confined myself to the report itself.

I should like to take this opportunity to thank Miss P. J. Westby for a valuable suggestion, and for her kindness in typing the manuscript.

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COMMENTS ON MR COLEMAN'S PAPER
BY DR DINGWALL, MRS GOLDNEY,
AND MR HALL

Through the kindness of the Editor of the *Journal* and with the permission of the author we have been allowed to see Mr Coleman's criticisms before publication, and are here presenting a few observations on some of those points raised by him which seem to us of sufficient interest to warrant a reply in print.

We would like first of all to thank Mr Coleman for the trouble he has taken in examining our report and for his help in indicating certain passages which perhaps needed further clarification.

We are inserting our replies under the paragraph headings used by Mr Coleman, or referring to the pages on which certain of his criticisms appear.

(a) ACCUSATIONS OF PRICE'S DELIBERATE TRICKERY

(i) *The Stone-throwing Incident*

We agree that a discrepancy is apparent between the statement of Mr Sutton himself and that attributed to him by Lord Charles Hope. It would seem from the evidence at our disposal that Mr Sutton did not actually see Price throw the stone but was so morally certain that Price had done so that, in recording his experience, he may have used to Lord Charles Hope a more precise description than the actual facts warranted. We regret not having discussed this point in our report but, in our view, it does not materially weaken the value of Mr Sutton's evidence.

(ii) *The 'Wine-to-Ink' Trick*

We do not follow Mr Coleman's question as to why KMG gave no first-hand account of this incident, since she is an author of the report and therefore shared responsibility for the description given on pp. 60-1. Had she not at the time agreed with the opinion quoted, i.e. that Mrs Foyster was responsible, we would have recorded this. But had she been told that the same incident had happened two years earlier when Price visited the Smiths at the Rectory, she might have reached a different conclusion.

With regard to our comment on Mr Foyster's testimony on p. 92, this seems to us to be amply justified.

Mr Foyster accepted as a phenomenon a smell of lavender in the bedroom. He recorded the discovery of a bag of lavender in the bedroom. A 'normal observer' would attribute the smell to

the physical presence of lavender. Mr Foyster did not ; he was not a 'normal observer'. He was a credulous man who was deeply infatuated with his young wife. He regarded both incidents as evidence that the house was haunted.

(iii) *The Smashed Vase*

We do not fully understand Mr Coleman's line of argument when he says that Mr Wall's statement does not rule out the possibility that the vase left standing on the upstairs mantelpiece was already smashed. Both Mr Price and Mr Wall state that the one thrown downstairs and smashed was one of a pair which was standing on the Blue Room mantelpiece upstairs just before. Nowhere does Mr Wall suggest that this other vase *had* also been smashed. Had it been inexplicably and paranormally smashed before Mr Price's arrival on the scene, would not the Smiths have commented on this fact when the second smashing occurred on 12 June and would not Mr Wall have included their account of the earlier occasion in his statement?

(b) CRITICISMS OF PRICE'S METHODS OF INVESTIGATION

(i-iii) We do not find it very easy to follow the argument outlined in these paragraphs. On p. 5 of the Report we tried to give our own view in the clearest possible terms and see no good reason for changing them. Our point was that Price gave suggestions to *uninformed* persons whom he chose for his observers with that fact in mind.

(c) CRITICISMS OF PRICE'S REPORTING

(i) *Bull Incumbencies*. Mr Coleman's principal complaint seems to be that insufficient trouble was taken to obtain explicit statements from Miss Ethel Bull. Miss Bull is a very old lady, approximately 90 at the present time. For reasons which are sufficiently obvious, we did not feel disposed to press her too far in her recollection of events which took place so many years ago. There was moreover a sharp difference of view between Miss Bull and her brother, the late Mr Alfred Bull, a complete sceptic as regards the hauntings, upon which it would have been necessary to dilate had we considered it desirable and charitable to criticise her testimony. Since the report was written this divergence has been given publicity in 'Is this the Borley Rectory Ghost?' in *Picture Post* of 1 January 1955. We suggest that it might be instructive for Mr Coleman to read again the testimony of Canon H. Lawton (p. 25).

There seems now to be no doubt that it was the Rev. *Harry Bull* who 'communed with the spirits' in the small summerhouse, and that it was Price's inaccurate reporting that attributed this pastime to the Rev. Henry Bull. In his article 'Ghosts, Cats and Moth-balls' in the *East Anglian Daily Times* of 15 March 1956 Mr J. O. Harley describes how, as a pupil of the Rev. Harry Bull, he shared these 'apparently sterile sessions' with the rector, and assisted him in the feeding of his twenty cats. This confirms the denial of their father's interest in the supernatural by the children of the Rev. Henry Bull reported by us (p. 4) and appears to resolve our own doubts on the subject (p. 21).

We take this opportunity of recording some further information regarding this period which has become available since the report was published. We have now obtained a copy of the article 'Mysteries of Borley Rectory' by Mr P. Shaw Jeffrey (*Cape Times*, January 1941). In this account of his experiences at Borley in 1885 and 1886 Mr Shaw Jeffrey states that the 'variety of disconcerting incidents' which happened to him at the rectory were attributed by him at the time 'to the practical joking of the junior members of this large family'. This comment, which is a striking confirmation of the assumption made by us as to the 'probable practical joking of the younger Bulls' (p. 27), was omitted by Price from the quotation of Mr Shaw Jeffrey's testimony (*EBR*, pp. 99-101).

(ii) We did not reprove Price for his 'omission of reference to the possibility of rose-bushes tapping in a "ghostly" fashion'. What Price omitted was the fact that they did so as reported to him by some of his observers. Suchappings could hardly have been reported as ghostly phenomena if their origin had been determined.

(v) We regret having overlooked mention of mice in Price's second book on Borley on one occasion by one pair of observers. Mr Coleman quotes us as saying that Price denied the presence of rodents in the rectory. This is correct. On p. 67 of our report we quoted many letters from Price's official observers commenting on mice, from several more left unquoted, which were in Price's files when he wrote (*MHH*, p. 62):

And as for rats or mice, during my investigation of the Rectory, on no occasion have I seen or heard the slightest indication of these rodents. And never once has any observer, to my knowledge, mentioned rats.

Can there be any doubt that this was a misleading statement?

(vi-ix) It is amply demonstrated by Price's private correspondence that he thought Mrs Foyster produced the 'phenomena' fraudulently (pp. 76-7), in contradistinction to the published opinion in his books that they were paranormal (p. 75). This was bad faith. It is not unreasonable to suspect the same motive in the matter of the omission from the later printings of the final sentence on p. 35 of the first edition of *Confessions of a Ghost Hunter*. The book was published during the period 1931-7 when Price displayed no interest in Borley, and in it he incautiously revealed his real opinion of Mrs Foyster. In subsequent editions during the period when he had decided to rent the house and advertise for observers he omitted the all-important sentence. Is it reasonable to suppose, as Mr Coleman suggests, that the deletion was made 'in case the Foysters took objection to it'? Why should Price have not feared that they might have taken exception to its inclusion in the first edition? Obviously after 1936 Price had decided that something could be made of Borley and was securing his flanks as far as possible.

Mr Foyster expressly stated (p. 90) that he had been told of the previous history of the hauntings. As the Foysters and the Smiths never met (p. 116), the probability seems overwhelming that the information came from Mr Foyster's own cousins, living only a mile or two from Borley. The partial quotation given by Mr Coleman from p. 116 offers no confirmation at all of Price's statement if the paragraph is read with any care. The point is that Mrs Foyster knew that objective phenomena were supposed to have occurred during the Smiths' time, and she might therefore well believe that the stage was safely set for her own activities. What she did not know, as she had never met the Smiths, was the opinion of Mrs Smith and others as to the genuineness or otherwise of the alleged poltergeists during the earlier period.

The suggestion that Mr Foyster's writings do not provide authoritative provenance of the wall-writings seems to us somewhat fantastic. All the wall-writings are described, both as to content and location, in the *Diary of Occurrences* written in 1931 (p. 111). They therefore occurred in 1931 at latest. Those observers therefore who first noticed them in 1937 and assumed that they appeared in that year were clearly mistaken (p. 112). It is true that Price mentioned the Amherst wall-writings in *EBR* after having claimed uniqueness for the Borley messages in *MHH*, and we regret not having notified the reader of this fact.

(xi) It is not easy to understand Mr Coleman's attitude towards the incident of the levitated brick. If Mrs Thompson was privileged

to witness the paranormal levitation of a $3\frac{1}{2}$ lb. brick at Borley, what possible motive can be suggested for her statement that it was thrown by a workman during the demolition of the rectory?

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Concerning Mr Coleman's comments on Mrs Smith we have, as he admits, set forth in much detail certain discrepancies in her 1949 testimony to us as against her letters and conversations at the time. She is a very religious woman and we formed the opinion that she was sincere. We had to form a judgment on the facts as a whole, and our judgment was that both she and her husband were subjected to and were particularly susceptible to suggestion; that the contradictions, and fluctuations of belief and doubt apparent even in her correspondence at the time, bore witness to the effects of this suggestion upon them. This view answers most of Mr Coleman's detailed objections.

The letter from KMG to Mrs Smith quoted by Mr Coleman is evidence of the fact that we gave our witnesses every encouragement to testify that they *did* believe in the haunt at the time. In this particular instance KMG was inviting Mrs Smith to produce testimony towards a haunt if she cared to reconsider her initial contrary testimony to us. The suggestion thus purposely applied was not towards the conclusions we ourselves favoured but away from them. We suggest Mr Coleman has misread the purport of the correspondence here.

We quote Mrs Smith's view about Mary and Mary's present view about the haunt. We give our judgment and readers are at liberty to form a contrary one if they so wish.

As regards the lights in the rectory windows, Mrs Smith's view was not the only one we quote in favour of a normal explanation. If these lights were in fact due to normal reflections, there is no inconsistency in statements that they were at times the reflections from passing trains, at other times the reflection from headlights on passing cars, again due to reflections from lights on the rectory landing, or from lights in the cottage (see Mr Henning's remarks quoted on p. 139 of the report). We gave the views expressed to us or given in Price's unpublished correspondence. In the absence of much better evidence for the haunting of Borley Rectory, we ourselves, like Mrs Smith, favour a normal explanation in preference to a paranormal one.

Page 256. The Testimony of Mr A. J. B. Robertson

As regards Mr Coleman's implied support of Mr A. J. B. Robertson's contention that it is 'of importance to consider the

sum total of all possible phenomena', it is of interest to notice the differing view expressed by Professor Antony Flew in his review of the report in the *Spectator* of 27 January 1956.

The whole story also shows the seductiveness of the Ten Leaky Buckets Fallacy, the unsound principle that though one leaky bucket will not hold water, maybe a row of ten will. Almost everyone connected with the Borley case seems to have been tempted to think that, even if the evidence was insufficient to prove paranormal agency in some particular part of the whole story to which they have had occasion to give close attention, nevertheless, if only you bring into the reckoning a lot more material not known to be superior the sum will add up to a genuine, sealed and certified haunt.

This is precisely the point which on p. 151 we endeavoured to make in regard to Mr Robertson's submission that the history of the hauntings as read by him in *MHH* was a factor properly to be taken into account in judging the possible paranormality of the phenomena experienced by his party, a submission with which we venture to disagree. It is of interest to note that in his first account of the results of the Cambridge investigation at Borley, Mr Robertson stated his opinion in the following words :

On the whole we must, I think, conclude that the evidence in favour of ghostly activity is not strong if we consider those results alone. . . . On the other hand, if the experiences of the Johnians are taken together with the whole of the previous evidence, the agreement is sufficiently striking to make one think that there are perhaps some grounds for ascribing some of the rather curious events described above to 'ghostly' activities. *The Eagle*, June 1943.

Page 257. The Canon of Carlisle and EBR

It was the late Mr Sidney Glanville who told one of us (THH) of his vivid recollection of Price's excitement on the receipt of the analysis by the Canon of Carlisle, and of his stated decision then and there to marshall the material for a second book on Borley. Price himself admits (*EBR*, p. 5) that it is doubtful if he would have attempted to write the book had it not been for Dr Phythian-Adams's great interest in the case.

Before concluding our brief replies to some of Mr Coleman's more important criticisms we should like to point out that on p. 74 of our report we invited readers to choose between three hypotheses to account for the happenings at Borley Rectory when Mr Price first appeared on the scene. Mr Coleman unfortunately does not tell us which of the three he personally favours. Until this initial decision is made, discussions on matters of detail are, perhaps, somewhat premature.

CENTENARY OF SIGMUND FREUD

ON Sunday, 6 May 1956, a simple ceremonial unveiling of a plaque on the wall of his house in Hampstead marked the hundredth anniversary of the birth of one of this Society's most distinguished Corresponding Members.

Sigmund Freud's association with the S.P.R. goes back a long way. Perhaps the earliest English writer to give public recognition of the importance of his studies was Frederic Myers. Writing in *Proceedings* in 1893 (Vol. ix, pp. 12-15), Myers discussed at length the recent paper by Freud and Breuer on 'The Psychical Mechanism of Hysterical Phenomena' and was pleased to find there some independent confirmation of his own views on the workings of unconscious mental processes.

Myers and the early psychical researchers engaged in pioneer studies of dreams, automatisms, and hypnotic trances because of a belief that paranormal abilities were somehow bound up with a part of mental life that is normally subconscious. Freud studied the same phenomena, but for him the fascination lay in the primitive and conflict-ridden human motives that became manifest once the reins of conscious, rational control were loosened. Nevertheless he appreciated the importance of those features which interested Myers.

In *Psychoanalysis and the Occult* (New York, 1953) George Devereux reproduced a large number of psychoanalytic papers dealing with parapsychology, among them six contributions by Freud, of which the one entitled 'Dreams and the Occult' that appears in the collection *New Introductory Lectures* (London, 1933) is perhaps the best known. While no one can doubt Freud's serious interest in parapsychology (witness, for example, his contribution to *Proceedings*, Vol. xxvi, pp. 312-18, entitled 'A Note on the Unconscious in Psycho-Analysis') he carefully refrains in his published writings from any categorical declaration of personal belief in telepathy. One hopes Dr Ernest Jones will give some more information in the third part of his Freud biography, but the important point is not Freud's personal belief or unbelief so much as the fact that he was prepared to examine the evidence and to consider telepathy as a legitimate scientific hypothesis. Such an open attitude does something to dispel the popular fallacy that Freud was irretrievably committed to a narrow form of nineteenth-century materialism. Above all he was an empiricist. Probably it was wise of him not to become committed to a definite judgment of the evidence collected by parapsychologists, for that is a specialist concern. In the matter of judging evidence in his

own field, Freud was well aware of the necessity for direct experience of the material.

Freud's major contribution to parapsychology was to point out that the characteristic features of subconscious processes as revealed by psychoanalysis should be found also in cases of telepathy. In his study 'Dreams and Telepathy' (*Collected Papers*, 4, 408-35) he remarks :

Nor does it embarrass me to be asked why I have made no use of the abundant supply of telepathic dreams that have been published. I should not have had far to seek, since the publications of the English as well as of the American Society for Psychical Research are accessible to me as a member of both societies. In all these communications no attempt is ever made to subject such dreams to analytic investigation, which would be our first interest in such cases.

In attempting to make up for this deficiency, Freud quoted various examples of apparently telepathic incidents among his own patients in which subsequent analysis rendered the occurrence more comprehensible. In one case, a father dreamed about the birth of twins and next morning received news that during the night his married daughter, who was not expecting her baby for another month, had given birth to twins. There was one flaw in the correspondence between dream and reality. In the dream it was the father's second wife and not his daughter who had the twins. In real life the man did not want to have a child by his second wife because he considered her unsuited to bringing up children. Freud hazarded the interpretation that the man was dissatisfied with his second wife and would have preferred a wife like his daughter. The dream construct, in which the daughter's and the wife's roles were reversed, was the result of a fusion of telepathic information concerning the daughter's twins with the unconscious wish to replace the wife with the daughter.

In another example, one of Freud's patients visited a fortune teller who asked for details about the birth of the person for whom she was to give prognostications. The patient gave the birth data of his brother-in-law and received the prophecy that 'This person will die in July or August of this year of poison from eating crabs or oysters.' The forecast did not come true, but it was a fact that the patient's brother-in-law was passionately fond of crabs and oysters and that some months previously he had been poisoned by some oysters and nearly died. Now Freud knew that this man had a strong fixation on his sister and unconsciously hated his brother-in-law as a rival. The medium's prophecy was utilising telepathically derived information about crabs and oysters to give

expression to her client's unconscious death wishes against his brother-in-law. As a result of noting other similar incidents with mediums, Freud suggested that mediumistic prophecies should be analysed as if they were the client's own dream or fantasy productions.

Freud limited his discussions to telepathy. Although he often had occasion to study spell-casting and magical rituals in general, and although he wrote a great deal about the superstitious compulsive rituals of obsessive neurotics, he never seems to have considered the possibility of active psi or PK occurring spontaneously in the clinical situation. That step has since been taken by some of his more venturesome followers.

The full weight of Freud's impact on parapsychology is yet to be felt, for it is only now coming to be generally recognised that no report of a spontaneous paranormal experience is complete without a very careful consideration of the psychological setting.

D. J. WEST

SOME PRESUMED PSYCHIC PHOTOGRAPHS —A FALSE TRAIL

BY THE REV. J. D. PEARCE-HIGGINS

IN September 1955 I had submitted to me a series of four transparencies taken at Stonehenge. The first was blank and dark, except for some strange images of what looked like candle-flames ; the next two of the series showed views of Stonehenge taken in broad daylight, one having ten and the other five candle-flames showing, in each case the top of the candle-stem (which was red) and the candle-flame showing ; and the last of the series showed a view without any candle-flame at all. The owner was at a loss to account for the presence of these candles on the otherwise excellent photographs of Stonehenge, which showed monuments with visitors walking about. He wondered whether there might be a psychic explanation.

He assured me that there had been no double exposure, and I concluded that the phenomenon might well be due to some fault in the processing, either during development or in the coating of the film with emulsion.

I submitted the transparencies to Mrs K. M. Goldney of the S.P.R. who kindly had them examined by an S.P.R. member, Mr F. Barlow, who is a photographic expert. He submitted them to Kodak Ltd, who reported on 11 October that 'the fault is in

no way connected with a processing defect', and suggested that there must somehow have been a double exposure. Mr Barlow was of the same opinion, and his verdict was, 'The candles and Stonehenge, when they appear on a single transparency (film) are proofs of a double exposure. Whether these exposures were made deliberately is presumably known only to the photographer.'

The films having been returned to me with these verdicts, in view of the owner's denial of double exposure, and my own conjecture proving false, I was at a loss to know what answer to give him to explain these rather strange phenomena which looked, on the lines of a psychic explanation, rather like the signs of some primitive rite or procession which had somehow got mixed up with twentieth-century events.

On 25 January I received, through a mutual friend, a letter from the owner which I quote :

It has occurred to me what is the true explanation of the phenomenon, which makes me feel extremely awkward about the false trail up which I have been leading you and others so long. It is undoubtedly a double exposure. I am afraid that it had completely slipped my memory that we had at home in the back garden . . . a Japanese 'suki-yaki' party. We were met in a circle round a primus stove which was cooking the meal, and as it got dark, candles were brought out. . . . I tried to take a flash picture of this, but the bulb didn't work. After trying two or three shots and failing, I decided I was wasting good colour film and turned back the roll for retake. But not one of the bulbs were any good. . . . When finally I gave up the whole thing as a bad job, I must have again rewound to the one following the original shot—you will remember there was one, the first was a pure 'night' scene [i.e. the dark one with candles only. J.D.P.-H.]. On top of these later I took the Stonehenge pictures, having completely failed to realise that the candles of the party were sufficiently brilliant to have left an impression on the sensitive film.

His further comment is interesting!

It surprises me that the S.P.R. has not announced the double exposure. [They had, of course, but he did not know for the reasons given below. J.D.P.-H.] Personally I have had no word from them or from P.-H. who promised to return the photos to me. I should have written before, but I confess to a secret desire to wait and see what the S.P.R. make of it. The fishy thing about the whole thing from the start is the modern look of the candles ; for an ancient representation they should have had a much more woolly flame. It is a let-down, but still it makes a good story.

The reason why I had not returned the photos before was (a) because the owner had gone abroad without leaving me his

address, and I should have had considerable trouble in finding it, and (b) because in view of the complete conflict between the S.P.R. report and the owner's assertions I was at a loss to know what to write to him. So I put it off. However, it is interesting and valuable to see how correct the S.P.R. report was, and also as a record of that human fallibility which makes psychic investigations so difficult.

INTERNATIONAL SYMPOSIUM ON PSYCHOLOGY AND PARAPSYCHOLOGY

UNDER the above title the fifth of a series of international meetings of parapsychologists sponsored by the Parapsychology Foundation of New York was held in the historic abbey at Royaumont, North of Paris, from 30 April to 4 May 1956. The chairmen on this occasion were Professor Emilio Servadio, an Italian psychoanalyst who has specialised in the analytic study of paranormal incidents, and Monsieur Robert Amadou, who is director of a psychic and mystical review *La Tour Saint-Jacques*. Doctors and psychologists made up the majority of the membership of the symposium; most were French, but nine foreigners attended. From the United States, Drs Jan Ehrenwald, Jules Eisenbud, and David Kahn, and also Mr Martin Ebon of the Parapsychology Foundation; from Italy, in addition to the chairman, Professor Martino, an anthropologist from the University of Rome; from Switzerland, Dr C. A. Meier, Director of the Jung Institute; from Holland, Mr George Zorab, who is now engaged on a study of spontaneous cases on an international scale; from Great Britain, Dr D. J. West of this Society.

As has been the case at all the conferences in this series, the technical arrangements for recording, translating and so forth were extremely well done. The opportunity afforded by the generosity of the Parapsychology Foundation for making useful personal contacts with foreign colleagues in comfortable surroundings was much appreciated by everyone. The papers and discussions largely dealt with the frontiers between parapsychology on the one hand and psycho-analysis and anthropology on the other. Speakers were more concerned with discussing new concepts and defining areas for co-operative investigation than in considering the raw data of the subject. Doubtless this is a correct approach for workers with a common background of experience in the field, but

those representatives of other disciplines who attended with a view to finding out what the subject was all about must have been puzzled.

For myself, I found Dr Ehrenwald's contribution 'Telepathic Leakage and Doctrinal Compliance' one of the most stimulating. He put forward the challenging suggestion, backed by historical illustrations, that patients are liable to use telepathy to produce psychological material to fit in with their doctors' preconceived theories. This was one implication of the telepathy hypothesis that Freud had not followed up.

D. J. WEST

REVIEWS

THE UNKNOWN—IS IT NEARER? By E. J. Dingwall and John Langdon-Davies. London, Cassell, 1956. 174 pp. 10s. 6d.

This book is written in a popular style, and is intended for the beginner in psychical research. The authors clearly believe in the efficacy of administering shock treatment at the outset. The introduction cites briefly a number of stories to illustrate the kinds of problem with which the reader will have to deal, and starts with the 'floating table' exploits of D. D. Home in 1856. Although those exploits remain unexplained, the student of exactly a century later is liable to discount such tales until someone else is able to perform comparable feats today, under really good conditions of control. The other stories in the Introduction are mostly from the records of this Society, and give a reasonably wide selection for illustrative purposes.

There are chapters on suggestion, experimental extrasensory perception, automatisms, telepathy and clairvoyance, precognition, dowsing and radiesthesia, apparitions and haunted houses, poltergeists, physical phenomena, and mental mediums. With so much ground to cover, there is naturally much compression. The amount of space devoted to each subject is not always proportionate to its importance. For instance, apparitions and haunted houses are dealt with in eight pages, whereas poltergeists and physical phenomena together occupy twenty-six pages.

The authors reserve their own opinions on the question whether paranormal material, mediumistic or other, furnishes evidence of survival. The book ends with a confident opinion that 'the Unknown is nearer, and is likely to be much nearer still in the not very distant future'. Such was the opinion of the founders of this

Society, but it is doubtful whether, if they were alive today, they would consider that progress had been as fast as they had expected it would be.

Considering the very insecure foundations on which many people base their beliefs in 'the Unknown', a book like this serves a useful purpose, even if it only leaves the reader with a wholesome realisation of the extreme difficulty of the subject.

G. W. L.

HEAVEN AND HELL. By Aldous Huxley. London, Chatto & Windus, 1956. 88 pp. 7s. 6d.

This is the companion volume to the well-known *Doors of Perception*, and to my mind it is the better of the two. The first book, it will be remembered, consisted of some fine descriptive passages of Huxley's own experiences under mescaline, of the recommendation that such experiences would be good for everyone, and of the correlation between the experiences of the mescaline-taker and those of religious saints and mystics. The present book starts off with some psycho-cosmology. The scenes and events witnessed under mescaline are regarded by Huxley as being completely autonomous and self-sufficient—as having an existence apart from our individual minds, the effect of the drug being merely to make us aware of them. This hypothesis is quite in keeping with the character of the visions and commits us only to a dualist theory of mind; which is nothing to be frightened of these days.

Huxley then speculates on the significance of the lack of colour in dreams and the abundance of colour in the mescaline phenomena. He relates this difference to the different symbolic content of the two modes of experience. This teleological explanation may be correct, but there is a more plausible physiological explanation to hand. This is that all monochromatic lights stimulate all three types of colour-sensitive cones in the retina to varying degrees, and so all 'natural' colours that can be seen are only poorly saturated, as they contain a large admixture of white. If, however, the two accessory types of cone could be inhibited, leaving the pure response of one type of cone, then colours far more saturated than any spectral colour could be obtained. It is probable that mescaline acts in this way. However, even if we can give a simple physiological explanation for the heightened colour of the mescaline phenomena, it is less easy to do so in the case of the improvement in *form* that occurs in single objects, or as manifested in the marvellous designs, tapestries, mosaics, landscapes,

etc. Huxley gives examples of these and compares them with the same, or at any rate very similar, phenomena that occur spontaneously, as described classically by George Russell, and in the accounts of heavens and fairylands to be found in religious and folk lore. Again it is not very implausible to suggest that these beliefs so widely held amongst primitive peoples have arisen to some extent from such experiences. But then Huxley proceeds to put forward the bold hypothesis that certain of our aesthetic interests, e.g. in precious stones, are determined by the presence in the nether reaches of the mind of, e.g., those visionary jewels so commonly encountered in mescaline and mystical experience. "The causal chain, I am convinced, begins in the psychological Other World of visionary experience, descends to earth and mounts again to the theological Other World of heaven." (pp. 24-5.) However, the presence of the visionary jewels in the mind must depend on ordinary sensory experience, for people who have never seen jewels adorn heaven with flowers; therefore the causal chain cannot *start* in the visionary world as Huxley claims. Rather we must suppose that the *particular* objects depicted are determined by ordinary sense-experience, but that their aesthetic quality is determined by the innate property of the hallucinatory sense-fields of the mind. There is another logical difficulty. Huxley supposes that we find jewels, gold, candle-light, etc., aesthetically compelling because similar things exist at the antipodes of our own minds and the sight of the former arouses our (unconscious) memories of the latter. But this does not explain why the visionary jewels, etc., should be aesthetically compelling in the first place. And if we could find any explanation for this, *this* explanation would surely also fit the real objects, and so in turn offer a better explanation of our aesthetic reaction to real objects, than merely to say that our perception of real objects is similar in various ways to the perception of visionary objects.

Huxley next proceeds to elaborate a new theory of art in some detail following these lines. This has some speculative interest but not much value as he has not explored the logical soundness of his premises with sufficient thoroughness. The last portion of the main section of the book is devoted to speculations on the posthumous state. It may certainly be claimed that the mescaline phenomena offer a plausible model of what our images may get up to if we continue to have images in any posthumous state, i.e. if Professor Price's theory of the next world is right—and if *this* theory is not right I am sure no other one can be.

The book is completed by a number of appendices, one of which gives a highly clinical account of the physiology of religious

mysticism leading to the very sensible observation that *all* our experiences are chemically conditioned, and thus we have no grounds for saying that experiences released by a chemical agent *cannot* have any human validity. Certain 'spontaneous' mystics have taken exception to Huxley's claim that a genuine mystical experience can be obtained by the use of mescaline. This, I imagine, stems from a mediaeval Paulian distrust of the flesh ; or from a local phobia of 'drugs' characteristic of Western culture ; or from the belief that only experiences that fit into certain theological systems may be held to be valid. The raw experiences induced by mescaline and occurring as the spontaneous phenomena are surely the same. Any differences between them can obtain perfectly adequate explanations in terms of the enormously different psychological and physiological conditions in which each occurs. It should not be forgotten that certain people—i.e. the nature mystics—do not interpret their mystical experiences in terms of current theology, or of a personal deity, and thus form a group intermediate between the mescaline group and the religious group. The poetical and aesthetic validity of the experience, both spontaneous and induced, is *given* in the experience. The psychological and theological *interpretation* of the experience is then up to the subject concerned. One might put the social position as follows. Mescaline has a valuable and increasing place in psychiatric therapy. It is also important that those professionally concerned with mind, such as psychologists and philosophers, should have had the experience in order to extend their acquaintance with the range of human experience. But certainly neither mescaline nor any similar agent should be used, on both medical and moral grounds, for the mere selfish enjoyment of the phenomena nor as a synthetic *soma*, as Huxley rather unfortunately suggested might be desirable in his first book on this subject. Of course, we cannot say that there *may* not be experiences called mystical of a type essentially different from the mescaline ones—i.e. a genuine direct communication with a personal God, rather than with the autonomous activity of our own minds ; although some people might equate the two.

The general thesis of the present book is, I am sure, correct and wise, except for the few minor points that I have referred to in this review—and, of course, it is written with incomparable style.

J. R. SMYTHIES

NOTES ON PERIODICALS

Oedème Bleu. By Ida Macalpine and Sir James Paterson Ross. *The Lancet*, 14 January 1956.

Everyone knows that hysteria can produce apparent loss of function of a limb or an eye, but fewer realize that faulty mental attitudes can also result in serious physical changes that are almost indistinguishable from those due to organic diseases. The authors of this paper describe and discuss two cases of *oedème bleu* (swelling, paralysis, and circulatory disturbance) that were due to emotional causes and which cleared up dramatically with psychotherapy.

The first patient, a woman aged 22, had severe and painful swelling and deep ulceration of the left arm which had been present for several years. The skin over the affected arm was unhealthily tight and shiny, and sensation and power of movement were both reduced. The patient wanted an amputation, and as penicillin and other treatments had failed she was referred to the surgeon for operation. Fortunately it was established that the reflexes, electrical reactions, lymph flow, and X-ray appearances were all normal, and the case was correctly diagnosed as one of nervous mimicry of organic disease.

It is important for those psychical researchers who are concerned with cases of alleged paranormal healing to be fully alive to the existence of hysterical cases that produce physical signs. Gross physical changes like these described in this paper can sometimes both appear and disappear as a result of psychological influences.

The Double: its Psycho-pathology and Psycho-physiology. By John Todd and Kenneth Dewhurst. *Journal of Nervous and Mental Disease*, Vol. 122, No. 1, July 1955, 47-55.

The authors discuss some possible causal factors in cases of visual hallucinations of the self. They differentiate between factors liable to encourage visual hallucinations in general (such as great facility for visualisation, dissolution processes in the central nervous system leading to the release of archetypal thinking, anxiety, fatigue, febrile-toxic states, alcoholism, epilepsy, and psychosis) and factors specifically related to hallucinations of the self. Among the specific factors the authors consider that narcissism—pathological interest in the self—and irritative lesions in the parietal lobe of the brain play an important part.

Psychical researchers and spiritualists have been particularly interested in those autoscopic hallucinations variously classified as 'out-of-the-body experiences' or 'experiences of the dying', in which a critically ill or anaesthetised person feels himself detached from his body and looking at himself from the outside. The authors do not discuss such cases as a separate category, but doubtless they would regard them as of similar causation. They quote one case of a woman who frequently had the feeling of a presence in the room of a person she knew. Sometimes it would dawn on her that the presence was none other than herself. The authors comment that 'it is significant than an obtrusive feeling of being in the presence of an invisible companion is a frequent experience of those subject to autoscopy'.

This paper is a tentative, pioneer exploration in a field that has been rather neglected by psychical researchers. Perhaps in future students of spontaneous cases will insist upon investigating the psychological characteristics and attitudes of all those who report hallucinatory experiences.

Notes on Parapsychology and Parapsychological Healing. By Michael Balint. *International Journal of Psycho-Analysis*, Vol. 36, 1955, Part 1.

Dr Balint begins with a cautionary illustration of the danger of too readily accepting at its face value an apparently telepathic incident when it occurs in a clinical situation. When he was first trying out the Szondi test, which involves presenting a series of pictures to the subject and asking him to specify his likes and dislikes, Dr Balint used two sisters as guinea-pigs. After the first of the two had made her selection she mentioned that it would be easy to imitate another person's choices. Dr Balint therefore challenged her to make the same selection as her elder sister would do. In the event her prediction proved to be extraordinarily accurate and very suggestive of ESP, but, as Dr Balint points out, the younger sister had probably closely studied the elder for years. A profound psychological identification might enable her to predict her sister's responses without resort to ESP.

Nevertheless, from his own and other therapists' experience, Dr Balint considers that instances of genuine telepathy do occur in the analytic situation. He thinks they are most apt to occur when a dependent patient in the throes of deep transference feels that the analyst's attention is straying. The telepathic material, which has the effect of taking the analyst by surprise, is motivated by the patient's desire to regain attention. Dr Balint believes that

the analyst also plays his part in the dynamic situation that builds up to a release of telepathy. The analyst's attempt to conceal his preoccupation with outside matters stimulates the patient to unmask the hypocrisy telepathically.

In Dr Balint's view the research worker in parapsychology may be psychodynamically involved in provoking ESP phenomena, just as the analyst is involved in the patient's telepathic display, but owing to his façade of objectivity the research worker may be less ready to admit it. On this point Dr Balint seems to be preaching to the converted, for the importance of the experimenter in the genesis of ESP results is being realised by the most 'objective' experimenters.

Dr Balint goes on to apply these interesting speculations to the question of paranormal healing. He notes that the authorities connected with the healing shrine at Lourdes lay down formal criteria for miraculous healing (e.g. instantaneous recovery, absence of a period of convalescence, definite physical change, etc.) which can never be applied to actual cases without all sorts of allowances and subterfuges being tolerated. All the same, he is impressed by the fact that practically every experienced doctor knows of unexpected and inexplicable recoveries similar to those reported in connection with Lourdes. He notes that the situation of the sick patient in mortal fear, and the doctor trying to conceal from the sick patient the limitations of his art, is very similar to the analytic situations that provoked telepathy. Perhaps certain physically sick patients may be stimulated in an analogous manner to produce 'paranormal' recoveries.

The chief criticism of this very interesting idea of Dr Balint's is that, unlike the reported telepathic incidents, actual cases of inexplicable cures have not so far been shown to be related to any particular psychodynamic situation. Argument by analogy is not a very safe scientific procedure, and in this case the two phenomena may not be at all analogous.

D. J. WEST

CORRESPONDENCE

POLTERGEISTS: SOME SIMPLE EXPERIMENTS AND TESTS

SIR,—I should like to make a few observations on the experiments and tests mentioned by Mr G. W. Lambert (*Journal*, 38, 201-11).

If the primary purpose of those experiments and tests is to

determine whether the movement of the walls and structure of a building may be the cause of the movement of physical objects within that building, the movement of any physical object unsecured to the walls and structure of that building is suspect. For this reason, but not for this reason only, I think that some of those experiments and tests are invalid. I suggest that it is essential to secure the glass container (mentioned by Mr G. W. Fisk) to the structure, and preferably to the inside of the outside walls of a building. The walls and structure of a building are not likely to move unless there is a normal explanation for their doing so. The walls of a building (for practical purposes) are always perpendicular. An outside wall may move more from the perpendicular (in the circumstances envisaged) than an inside wall. If the container is placed on or secured to an object within the building, and that object is not secured to the structure of the building, it is possible that the container and/or that object may be moved other than by the movement of the structure of the building. Such a test is inconclusive.

I am told that a displacement of 6" (and probably less) from the perpendicular in a building of normal construction and 48' in height would cause visible damage to the structure. A pendulum of 1' in length secured to the wall of such a building would show a displacement from the perpendicular of $\frac{1}{8}$ ". If the length and width of such a building were 30' (they might be more) and if the walls and floors remained at right angles to one another, would an elevation of 6" or thereabouts of one end of those floors over a surface of 900 square feet suffice to explain the movement of physical objects? Unless the walls of the building are shaken, I do not think that the movement of liquid is likely to be detected with any degree of accuracy unless the most delicate instruments are used.

The ideal test, perhaps—if only that were possible—would be that which was conducted in a building of several storeys. To establish audible and/or visible evidence of the moment and extent of movement from the perpendicular, electrical equipment could be devised consisting of a number of metal pendulums (all of the same length and say 2'), four of which would be secured to the inside of the outside walls of each storey and on opposite sides of the building to one another, each pendulum being suspended within a circular and metal container (the container being encased and hermetically sealed to prevent interference with its contents by any known means) and separated from their containers with which they would make contact, by distances of e.g. $\frac{1}{8}$ ", $\frac{1}{4}$ " and $\frac{1}{2}$ ". If contacts were made and recorded at such distances, they would

indicate a displacement from the perpendicular of 3", 6" and 12" respectively. I believe that tests conducted in such a manner might prove conclusive.

DENIS CHESTERS

Brighton,
Sussex.

SIR,—Are we really supposed to take Mr Lambert's articles seriously? No doubt some phenomena may be capable of the explanations which he proposes, but not all! Surely saucepans do not rise up off tables and empty themselves over the shoulders of highly respectable matrons and assistant matrons of a nursing home, as happened during the Poltergeist phenomena at the Nursing Home in M——; nor do pokers detach themselves from the wall and float up into the air in the direction of the object and also agent(?) of the poltergeist, in the presence of highly respectable witnesses such as a doctor and the nursing home staff, just because the wall of a house has tilted to a microscopic extent as the result of subterranean waters! Nor can I be personally persuaded that the three weeks' phenomenon of rapping heard by myself and my family on the ceiling of the Cornish Rectory of St Gerrans (in August 1948) which I tested and probed with great care in the endeavour to find a naturalistic explanation, were due to movements of the house. Presumably some of the observers of the poltergeist phenomena would also observe the movements of the walls, etc., if that were the real cause! Mr Lambert's explanations are not science, they are pure prejudice, as is, I fear so much of the argument advanced by some of those who gain publicity in the *S.P.R. Journal*. They seem all working to a quite simple and most perilous formula which works out thus :

1. There must be a mechanistic or naturalistic explanation for all so-called paranormal phenomena.
2. If no naturalistic explanation can be found to fit the case, then it is clearly a case of mal-observation by the observers.
3. All human testimony which conflicts with a naturalistic explanation is clearly faulty.

This is neither science nor common sense, and brings discredit on the study of the subject, and certainly little credit, or credibility to the authors. The psychology of scientific credulity seems subject to the same laws as those of unscientific credulity, viz. wishful thinking carried to excess.

J. D. PEARCE-HIGGINS

London, S.W. 15.

SIR,—I have read with interest Mr Lambert's article on Poltergeists in the *March Journal*, in which he expresses the view that house-displacements may have caused many of the phenomena recorded. Now, I have lived in California for the past twelve years, and during that time I have lived through a number of minor earthquake disturbances, and three major ones. In the second one, particularly, china and windows rattled, and a large book-case, some nine by seven feet, filled with books, swayed lengthwise back and forth. But in no case was a single small object, resting on shelf or mantelpiece, displaced, nor did it fall to the floor—far less was it thrown across the room! If a relatively severe earthquake will not produce such effects, can we seriously believe that the slight settling of a house will do so? Surely this is straining at the gnat while swallowing the camel!

Finally, I cannot see why thinking that poltergeist phenomena may be due to the presence of an 'unconscious medium' should produce any more 'mischievous effects' on the individual holding this view than thinking that they were fraudulently produced in an unconscious state, or while suffering from repressed emotions or partial dissociation. One theory is as good (and as impersonal) as the other. Why then should one view be any more 'detrimental' than the other?

HEREWARD CARRINGTON

Hollywood,
California.

Mr G. W. Lambert has furnished the following comments on the above:

It is admitted that neither of the two tests suggested is ideal or necessarily conclusive. Their chief advantage is that the proposed apparatus can be installed quickly, while there is some hope of the phenomena continuing. An ideal apparatus, which is always liable to arrive too late, would probably not be worth the money spent on it.

It is envisaged that in many cases the whole site on which a house is standing is tilted. In that event, assuming a slow and even tilt, with no unequal strain on joints, a house could probably be tilted some distance from the perpendicular without visible damage. Ordinary experience with single walls that have got out of plumb is no guide. A well-built house, tied together at the top by the roof timbers, would presumably behave much like a box firmly anchored to the ground.

The container in the 'Beaker test' need not be anchored to

the floor or table, unless very violent movements are anticipated. As mentioned in the instructions, care should be taken to exclude unwanted interference. Here again, the object is to recommend practical measures which almost any householder could carry out at short notice.

Mr Pearce-Higgins's letter probably represents the views of a considerable number of readers, and I am glad to have this opportunity to comment on it.

If, as he is prepared to admit, my working hypothesis explains some poltergeist phenomena, it must be taken seriously, as it is not clear how much ground the theory covers. In the last paragraph of my first paper on this subject in the *Journal* for June 1955 (38, 49-71), I warned the reader against the supposition that the theory would explain all cases of the kind. Whether in a given case it explains all or some of the particular incidents is a question of evidence. In many cases the evidence available is insufficient to enable a confident conclusion to be drawn. In others it is fairly clear that some of the incidents are due to physical causes, while others are due to psychological or parapsychological causes. I would class as a 'mixed' case the one relating to events at Ardachie Lodge reported in the *Journal* for December 1955 (38, 159-72).

About the events in the Nursing Home to which Mr Pearce-Higgins refers, he has kindly furnished some details, additional to those which were already in the possession of the Society. Most of the occurrences seem to have taken place in the kitchen, and were well attested. They were of the following descriptions :

- (1) cracking of milk bottles on the kitchen floor ;
- (2) scattering of milk out of a large basin ;
- (3) flying of plates off a dresser ;
- (4) tipping up of saucepans ;
- (5) 'antics' of a swill tidy ;
- (6) jumping of a poker off a nail in the wall on which it was hanging.

All the above 'pranks' could have been effected quite easily by underground water. If the kitchen floor was of flagstones, it must be observed that they are particularly liable to be jerked up and down, with mischievous results to stoves and dressers, and any utensils or crockery standing on them. The water, being all the time well below the floor, is not seen, and the cause of the commotion is not detected.

The town in which the Nursing Home is situated is at the foot

of a high hill, and there are copious springs in the neighbourhood.

To account for the mysterious noises heard in the Cornish rectory, there is no need to postulate any movement of the house. They could have originated underground, and have been conducted up into the building. An examination of the house, however careful, would not discover the place they came from. The Rectory of St Gerrans Church is shown on the 6-inch Geological Survey map as standing on the 'neck' of a small peninsula, jutting southwards into Falmouth Bay. On one side the open sea is less than half a mile away, and on the other, at about the same distance, there is a tidal creek. Probably the times of high water are not the same on each side. In such a case the tides not only depress the peninsula in the manner described by Professor G. H. Darwin (*Journal*, 38, 56), but also wrench it from side to side. The result is that cracks and fissures develop at the neck, along which water can move, whether it is rain water from above, or sea water from below. In the end the peninsula is bitten right off by the sea, and an island or isolated rock is left behind. One hopes that fate is a long way off for St Gerrans, but the mysterious noises are probably a matter for the geologist rather than for the student of psychical research. The rock is blue slate, and it would be difficult to find a more favourable spot for a tidal poltergeist.

Mr Pearce-Higgins attributes my views to prejudice, and assumes that I apply to my research what he rightly calls 'a most perilous formula'. Clearly he has not read earlier papers by me in the *Journal*, on the Dieppe Raid Case (36, 607-18), and a reinterpretation of the well-known Versailles Case under the title of 'Antoine Richard's Garden'. In both those cases I gave at length reasons for thinking that 'naturalistic' explanations would not fit the facts. As I do not subscribe to the formula which Mr Pearce-Higgins criticises, I can readily agree with him as to the danger of wishful thinking in this connection. I had hoped to avoid the charge of indulging in it by giving full information as to my sources of information and lines of reasoning.

Mr Hereward Carrington has a much more profound knowledge of the history of poltergeists than I have, and his experience of earthquakes is very interesting. With diffidence, I venture to doubt whether earthquakes would bring about the same effects as 'poltergeist' movements due to water. In the former case the wave is a long one, compared with the longest axis of the house. One may compare the building to a small boat at anchor, riding up and down on a long ocean swell on a fine day. There is no pitching or rolling, as the boat is lifted bodily. The force of under-

ground water, however, is usually applied at a relatively shallow level, and may be confined to one quite small area. It seems to be able to administer a series of blows rather like a severe water hammer, so familiar in domestic water pipes. In this matter an ounce of experience is worth a ton of theory, and I would like once more to remind the reader of the experience of the reporter in the Portland (Oregon) case (*Journal*, 38, 60). That was not so very far from where Mr Carrington lives.

The mischievous effect of the 'unconscious medium' theory is not upon the person holding it, but upon the unfortunate person who is suspected (quite wrongly, in my opinion) of being the unconscious medium. In these days the chief danger is undeserved publicity, which 'spoils' the victim of it.

ESP DURING SLEEP?

SIR,—When I want to wake up at a certain time I often do so as long as there is a clock in the room. Sometimes, however, I have found myself waking at, say, 12.20 instead of the intended time of 4 o'clock. This puzzled me until it struck me that although the actual time did not correspond, the angle between the hands of the clock did. From this I concluded that there is probably a connection between waking up and the clock. To test this I have used a clock that shows an unknown time (by covering its face with a metal lid, and turning the hand an unknown amount, and taking off the lid in the dark). There still was a frequent correspondence between the mentally set target and the time or angle shown on this clock. My conclusion was that I observe the clock while I am asleep. In an effort to get more exact data with a view of getting statistical proof, I devised a test which is described hereunder. A run of 200 observations with this test gave the following results :

49 times (1 in 4) there was a correspondence between the angles of observed time and target time to within ± 2 minutes (expectation 1 in 8).

In 17 cases (1 in 12) there was a correspondence between observed time and target time to within ± 2 minutes (expectation 1 in 114).

I realise that my data are insufficient to have any statistical value. That is my reason for writing this short report in the hope that interested readers might try out the experiment. I do not think the observations I made are in any way exceptional ; on the contrary, I have met a great many people who admit the same ability to wake up at will.

Description of test : Two clocks (alarm clocks for cheapness) are fixed side by side in a box, the first one not wound, the second one going. The second clock can be stopped by pulling a string. The target time is set on the first clock by covering its face and turning the hands an unknown amount. The box is then closed and put under the bed. Pull the string when waking up, and write down the result in the morning.

My tests all took place between 22.54 hrs. and 6.33 hrs. for mathematical reasons.

W. VAN VUURDE

P.O. Walkerville,
Transvaal,
South Africa.

A NEW PARAPSYCHOLOGY LABORATORY

As we go to press, news has been received of a grant from the Parapsychology Foundation, New York, to Dr Carroll B. Nash for the establishment of a laboratory for research in parapsychology at Saint Joseph's College, Philadelphia, U.S.A., where Dr Nash is Professor of Biology. Further details will be given in the next issue.

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